THE SIR WILLIAM MACLEAY MEMORIAL LECTURE 1984

New Light on the Extinction of the Australian Megafauna

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Coming as I do from the University of Sydney I frequently encounter the name of Sir William Macleay. The energies of the staff of the Macleay Museum ensure that his name is kept in front of us.

The subject of Oceanic prehistoric archaeology has an important place within the Macleay Museum. Thus my sort of archaeology is, within the University of Sydney, associated with the name of Macleay. By contrast archaeology has not been so associated, at least in this century, with your Society. I have looked at the contents of your *Proceedings* and suspect that mine may be the first contribution in prehistoric archaeology for some eighty years. I therefore feel that it is a double honour for me to be giving the Sir William Macleay Memorial Lecture to your Society. The honour is both a personal one, and one for the discipline of prehistoric archaeology within Australia.

Of course, much of Australian archaeology is not relevant to the aims of your Society. I would not expect you, for instance, to find a place for research into the manufacturing sequence of stone tools, or a study of their functions. However the study of megafaunal extinctions, incorporating as it does such studies as zoology and botany, is clearly relevant to your Society.

Extinctions is so manifestly a topic of what used to be called natural history that some of you may be wondering why I should be claiming it as also a topic for study by archaeologists. Let me give you two reasons why it should be. The first reason is chronological, and the second methodological. The chronological evidence that megafauna and prehistoric humans overlap is increasing. The period of overlap was evidently some tens of millennia. The methodical reason, related to the chronological one, is that archaeologists, in their approach to studying past events in the field, are accustomed to paying minute attention to stratigraphic detail. Of course such attention is not restricted to prehistorians: indeed we have recently seen some palaeontologists, examining the extinction of dinosaurs at the end of the Cretaceous, reducing their research objective to the study of the chemical properties of a band of dust 10mm thick. Nevertheless, though they do not possess a technique peculiar to themselves, archaeologists have a strong tradition of relishing the fine detail of the context in which their specimens are found, as much as they relish the specimens. We do not dig for specimens as we might dig for potatoes.

Field work designed to establish the relationship between humans and megafauna will always require fine attention to contextual detail. From this point of view it is fortunate that the question of Pleistocene extinctions, in both the Americas and Australia, is being answered by the use of archaeological methods.

It has not always been thought that the question of Pleistocene extinctions in Australia was one for archaeologists. Until a decade ago in Australia one could respectably argue, though with some opposition, that megafaunal extinctions pre-dated the first settlement of this continent by the ancestors of the Australian Aborigines. Where were the archaeological sites, with unambiguous stratigraphy and radiocarbon dates, at which the sceptical excavator could repeatedly find artifacts and megafauna in association? After decades of armchair speculation and (I would say) desultory field work there were none. Indeed it was questionable whether there was any overlap in time. For example a Holocene date for a tooth of *Diprotodon* from Orrorroo, in South Australia, was about 7,000 years old: less frequently reported, by the supporters of recent megafaunal extinctions, was a date of greater than 40,000 years for the contents of the stomach of the same beast (Jones, 1968). Another critical site, for the adherents of early extinctions, was Lake Mungo. Archaeologists, studying in detail Lake Mungo's artifacts and food remains at stratified locales older than 20,000 years, were absolutely unable to find any extinct species (Bowler *et al.*, 1970).

Yet in spite of its attractiveness an extinction that pre-dated human arrival was not a view that was strongly argued, largely because it left unanswered the question of what else might have caused the extinctions. So a different explanation came into favour. Rhys Jones (influenced by Paul Martin, that doyen of the American debate about extinctions) provided us with a respectable hypothesis of human causation. The argument that gained strength in the 1960s was an extinction caused by the initial human settlement of Australia towards the end of the Pleistocene (Jones, 1968). It was thought to be sudden and ecologically catastrophic.

There were several observations that made plausible the hypothesis of extinctions soon after first human settlement:

1. Species stable since the Tertiary (notably *Diprotodon*, short-faced macropods such as *Procoptodon* and *Sthenurus*, and the carnivorous phalanger *Thylacoleo*) suddenly became extinct at the end of the Pleistocene. There might be room for argument about how synchronous the extinction of the various species was, but by the vast scale of Cainozoic chronostratigraphy there could be no doubt that the extinctions were sudden.
2. The species that became suddenly extinct had survived numerous climatic oscillations, of precipitation and temperature, that had earlier taken place in the 2-3 million years of the Pleistocene. They not only survived these oscillations but, by their fossil distribution across latitudes and coastal-inland gradients, showed themselves not to be sensitively adapted to specialized environmental conditions. They were, in other words, species tolerant of diverse environmental conditions, in the same way that the grey kangaroo is tolerant. The largest species (those allied to the genera of *Diprotodon* and *Zygomaturus*) have been collected as fossils from Papua-New Guinea to Tasmania, and out west to the lakes of the Lake Eyre basin. Even allowing for diachronic variations in palaeoclimates it would be hard to argue that *Diprotodon* was anything but a very tolerant animal.
3. While not precisely knowing the habits of the extinct megafauna it is possible, from a study of the functional anatomy of the teeth, to identify the majority as browsers. Of especial interest was *Diprotodon*, evidently a riverine browser. *Thylacoleo* was plausibly interpreted in a quite different way, namely as a leopard-sized carnivore. In these two cases (and in others) species became extinct in niches that were then left unfilled. The principle of competitive exclusion could not be invoked as a cause of extinction.
4. It was argued that climate could also not be invoked. Palaeoclimatologists the world over had not detected, towards the end of the Pleistocene, a climatic event that was novel. True, we saw the severely cold and dry pleniglacial event,

centred on 18,000 years ago, with particular clarity, but we saw it in that way because it was so recent and had left so complete a record of its effects. However even a cursory study of older landforms indicated that events just as severe had been recurring events in the Australian Pleistocene.

5. There was only one discernibly novel event at the end of the Pleistocene in Australia, namely the arrival of human beings. The question was whether we were to see them as ecologically neutral or as analogous to a new disease.

All the considerations outlined above applied to the Americas as well, where ground sloth and mastodon became extinct at the end of the Pleistocene. Again we know that it was at that time that humans arrived.

Thus developed the idea of the extinction of a naive fauna faced by a novel predator in the form of humans. The fauna was thought to be naive in the sense manifested by animals that live today in areas not previously occupied by humans, such as certain ocean islands, areas of the Arctic and all of the Antarctic. The absence of humans in those places has meant that the animals evolved no behaviour specific to humans as predators.

In 1973 Martin, writing about the Americas, sharpened up the general theory of human predation to a specific one. He argued that newly-arrived humans, and their descendants, formed an advancing predatory front. This front had become adapted to the naive resource of megafauna. Martin argued that the first Americans swept the Western Hemisphere and killed off the megafauna (the largest species being the most economical part, for such a human adaptation, of the total mammalian biomass) within 1,000 years (Martin, 1973). This new and extreme theory for the precise mechanism of extinction fitted with the fact the radiocarbon dates in Tierra del Fuego were as early as dates from the north of North America, namely around 11,000 years old.

Though not explicitly extended to Australia, this blitzkrieg model attracted Australian archaeologists. Here too dates in the south were as early as dates in the north, and no sites associating artifacts and megafauna, in reliable and repeatedly observable stratigraphic association, had been found.

It is perhaps not immediately obvious to you why the absence of blitzkrieg sites was expected by the proponents of this model for megafaunal extinctions. The answer is (and it is a reasonable one, even though it might be thought to retain overtones of special pleading) that in terms of the probability of discovery we cannot expect to find the short-lived blitzkrieg event materialized in the archaeological record. After all, and on a vastly broader timescale, millions of individual *Diprotodon* must have lived in New South Wales alone, yet the specimen from Tambar Springs is the only individual for which we have more than half the bones in the skeleton. Indeed we can note in passing that when the blitzkrieg argument was being put together we did not have even the Tambar Springs specimen. This marvellously unique skeleton, that is now on display in the Australian Museum, was found only in 1979. The argument in brief is that since we have so few fossils, compared with the individuals that lived, why should we expect to be fortunate enough to find those dating from the short-lived blitzkrieg. The implications of the blitzkrieg model for field research are depressing.

My field research into the question of megafaunal extinctions did not start until 1974, well after the opposing armies of climatic and humanly-induced extinctions had taken to the field. Had I earlier joined the theoretical fray I have no doubt that I would have favoured the arguments for humanly-induced extinction. I was certainly attracted by the bold explanatory power of the blitzkrieg model.

I now have to stress that, regardless of its theoretical appeal, the blitzkrieg model for Pleistocene extinctions is seriously threatened by the stratigraphic evidence emerging from the archaeological sites that I am going to make the subject of the rest of this lecture.

In order to set this current field work in context I shall briefly describe some earlier excavations carried out, by myself and several colleagues, near Melbourne in the 1970s (Gillespie *et al.*, 1978). At the site of Lancefield we were able to date the megafauna to 26,000 years ago. This is a date not for extinction itself, but rather for a still-extant megafauna. Extinction in Victoria took place at some still unknown date after the deposits at Lancefield were laid down.

Now a date of 26,000 years ago is a suspiciously late date if the blitzkrieg model is to hold. We knew before we dated Lancefield that human occupation at Lake Mungo was at least 34,000 years old. Furthermore after we published Lancefield we were to learn that there is, for the Western Australian site of Upper Swan, reliable evidence for humans at, or earlier than, 40,000 years ago (Pearce and Barbetti, 1981). By 'reliable' I mean stones that are irrefutably artifacts, *in situ*, and with repeated radiocarbon dates.

In brief, then, a period of several thousand years of overlap between humans and megafauna was fatal for the blitzkrieg model. Yet the implications of Lancefield went further than the mere overlap of radiocarbon dates with archaeological sites elsewhere. We found at Lancefield two stone artifacts that could be stratigraphically related to the bone bed — one, under conditions of impeccable control, was found in a little channel under the bone bed and was therefore earlier than an occurrence of megafauna on grounds of relative dating alone.

Though it was (and still is) a site that seriously challenges the blitzkrieg model, Lancefield had a severe deficiency. It had no extended stratified sequence in which artifacts and megafauna could be found in association in successive layers. The deposits at Lancefield happened during a short period around 26,000 years ago when the spring-fed swampy depression was filled with shallow water. The depression filled up and, as the pollen record shows, no more free water was available. There was therefore no attraction for animals, or rather there were no depositional processes that preserved their visitations. So the archaeologist at Lancefield excavates down through two metres of recent deposits of sandy clay, across an unconformity and into the bone bed. Importantly the charcoal used for dating was recovered from just under the undisturbed bone bed, with (as I have already mentioned) an artifact. So in spite of the absence of a continuous association I found the stratification beyond reproach as regards the late date for megafauna. Furthermore the two dates of 26,000 years were obtained from separate parts of the site. Nevertheless we did depend entirely on the reliability of the two radiocarbon dates for the recent age of the megafauna, and we had no continuous sequence, associating humans and megafauna, that could be dated from top to bottom and provide some chronological history of the association.

One might have supposed that a site with better stratigraphic properties than Lancefield would have been easy to find. Yet it has been the case that since we published our findings the discovery of a stratified sequence on the mainland of Australia has proved elusive, but, if the dates from Lancefield are correct, the discovery of such a stratified sequence is to be expected.

As an improvement on our work at Lancefield our recent work on the Liverpool Plains of New South Wales looks encouraging. It is to this work that I shall now turn. The Tambar Springs Project, as it is informally called, has for the first time provided direct stratigraphic evidence for a prolonged and continuous overlap of humans and megafauna at a single site. Thereby the evidence from Lancefield has been markedly improved upon.

In providing you with an account of the Tambar Springs Project I stress that there are many loose ends to the work so far done. This lecture is therefore a report on work in progress. It is also a report on work done in collaboration with others, currently David Horton (of the Australian Institute of Aboriginal Studies) and Judith Fethney of the University of Sydney. Students of the University of Sydney have provided the labour that these labour-intensive sites require. The work has been supported in part by the Australian Research Grants Scheme.

We have been excavating at two sites on the Liverpool Plains, called Lime Springs and Trinkey. Both sites are spring-fed swamps, unimpressive depressions in the landscape. Each is less than 100m in diameter. Neither is identifiable as a site on aerial photographs, yet their unimpressive appearance belies their scientific importance. We discovered both sites by ground survey, and into the banks of the swampy depressions we dug blind, since no archaeological evidence outcrops on the surface.

The sites are 3km apart. Both have essentially the same stratigraphy. The prehistoric stratigraphic units, from the top down, are: —

1. The Grey Silt, an aeolian dust dating to 6,000 years old: up to 0.8m thick.
2. The Black Swamp, an organic-rich black sandy clay dating from 6,000-c. 20,000 years old: up to 1.3m thick.
3. The Buff Silt, an aeolian dust c. 26,000 years old: up to 0.5m thick.

In substantiating a recent date of megafaunal extinctions it is the Black Swamp that holds the clues. Wherever we have dug, and at both sites, the Black Swamp contains stone artifacts and megafauna. All the mammalian remains (both extinct and extant species) have been identified from fragments of the enamel of their teeth. Bits of bone are fragmented, and nearly one third are burned, suggesting that they are the remains of human activity around the spring-fed swamps.

At Lime Springs (by far the richer of the two sites) extinct species present include *Diprotodon*, *Macropus titan*, *Protemnodon*, *Procoptodon* and *Sthenurus* (Gorecki *et al.*, 1984:118). In the same levels we found thousands of artifacts, including 1988 that were greater than 10mm in minimum dimension. From the point of view of the age of extinctions it is critical to note that the upper levels of the Black Swamp at Lime Springs (dated by analogy with Trinkey to 6,000 years old) have as much extinct fauna as the lower levels.

When we published the site of Lime Springs we had only one date, and that was for the base of the Black Swamp unit. Now we have several dates and they are all consistent.

We have the remarkable evidence of the megafauna living through to the Holocene. As my late mentor Louis Leakey used to say, the textbooks will have to be rewritten. Indeed a Holocene megafauna is not the only remarkable attribute of the Black Swamp unit at both sites, since we also find high groundwater discharge from the springs through the pleniglacial period centred on 18,000 years ago and which, to the south and west of the Liverpool Plains, has been shown to be exceptionally arid. Another unexpected discovery is cultural: at the top of the Black Swamp (and therefore dating to about 6,000 years ago) we find the first occurrence at our sites of the horsehoof cores of the Kartan industry. This industry (previously undated with any precision) is found in arid areas to the west; the Liverpool Plains represent the most easterly occurrence of this fascinating prehistoric industry (Lampert, 1983). I will comment again on the significance of the Kartan industry when I describe the uppermost unit called the Grey Silt.

The sandy clays of the Black Swamp unit, though below the water-table, retain well-differentiated cultural stratigraphy. We were able to use correspondence factor analysis of the excavated units, taking as data the counts of the rock types used to make artifacts, to demonstrate that the site was not disturbed (Gorecki *et al.*, 1981:119). Since, in addition, we cannot differentiate the state of preservation of extant and extinct species we have very strong *prima facie* evidence for a Holocene megafauna on the Liverpool Plains.

These two sites are better than Lancefield for the simple reason that each has a sequence, covering some 14,000 years, during which artifacts and megafauna are associated. For the first time claims for coexistence do not depend on casual and unrepeatable observation. It is possible to invite a sceptic to excavate a square metre and guarantee that the association will reveal itself again. Repeatability is one of the essentials of good science.

It is important to note that our findings, surprising though they are, do not fly in the face of findings from other archaeological sites on the central and western slopes. There are no other sites yet excavated. True, extinctions in the fragile ecosystems of the lakes in western New South Wales were complete by 27,000 years ago (Hope *et al.*, 1983). Yet we must remember that our area is (today at any rate) so well-watered as not to be so easily disturbed by climate or humans. Let us consider an environmental parallel. Giraffes and rhinoceroses went extinct in the Sahara following minor climatic changes, but flourish to this day in the well-watered Serengeti. We do not think of African environments in a monistic way, yet we tend to fall victim to the cliche of Australia being the driest continent. The driest continent it may well be, but this statement of average overlooks the hundreds of thousands of fertile square kilometres in the well-watered east. My point is that the fragile ecosystems to the west give us no reason to predict synchronicity of extinctions in the different ecosystems of the east. The only theory that requires synchronicity is blitzkrieg: Lancefield, Lime Springs and Trinkey indicate that continent-wide extinctions are not attributable to initial human impact.

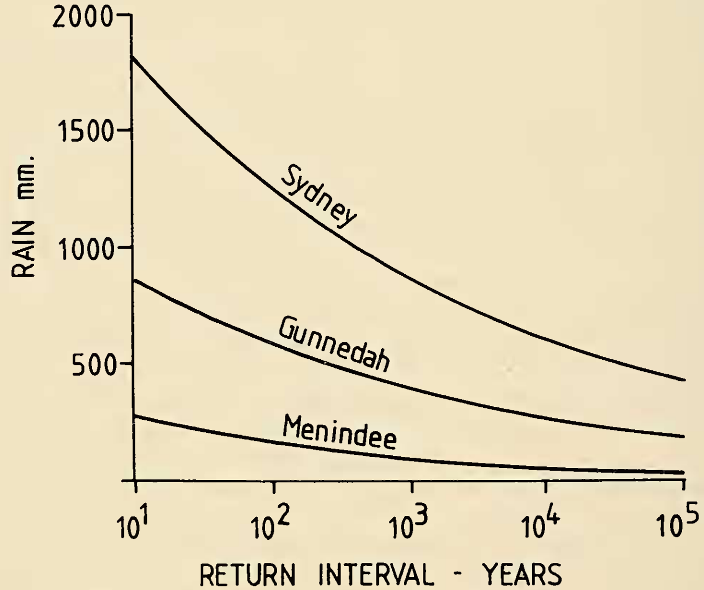

Fig. 1. Return intervals of droughts, computed from the rain falling in Year *J* + Year *J*-1. Gunnedah, on the Liverpool Plains is proofed against drought in a way that Menindee, representing the western lakes of New South Wales, is not.

We published (Gorecki *et al.*, 1984:117) a graph showing the theoretical importance, when studying extinctions, of summer rainfall on the Liverpool Plains. Fig. 1 extends this line of thinking into the statistics of the return intervals of droughts. The graph is calculated from the rainfall statistics of the last hundred years (in lieu of statistics from the Pleistocene!). The algorithms for computing return intervals are taken from

Gumbel (1958). To make the statistics more ecologically meaningful I have computed the return intervals not for annual rainfall but for the rainfall of Year *J* + Year *J*–1. Thus we are looking at the return intervals of droughts calculated from the sum of two year's rainfall.

How can we make use of Fig. 1 to illustrate the stability of the Liverpool Plains? Let us suppose that we assume a total of only 200mm of rainfall, spread over two years, as locally catastrophic to large mammals and hunter-gatherer humans in any environment. Fig. 1 indicates that the average return interval of such an amount at Menindee is in the order of once every one hundred years. At Gunnedah the return interval exceeds one hundred thousand years. Thus we can see that the Liverpool Plains not only have better absolute rainfall than the western lakes (including a critical peak of summer rainfall) but they are far better proofed against crippling droughts.

The enduring stability of the Black Swamp unit was destroyed 6,000 years ago when the Grey Silt was blown in. This remarkable unit is best represented at Trinkey, where nearly a metre of calcareous aeolian dust sits conformably on the Black Swamp unit. At Lime Springs, where the silt lies below the water-table, it has evidently been decalcified so that only the siliceous component remains. The deposition of Grey Silt represents a rude interruption to the swampy ecosystem since no more swamp deposits formed until post-European times, when the clearing of the land caused a rise in the water-table.

This calcareous dust is foreign to the area. Certainly it is not a source-bordering dune, since weathering products of the Pilliga Sandstone surround the two swamps. Moreover the Grey Silt is found at its deepest within the Trinkey depression and to the west, not as a dune on the eastern margin.

The theory that I am working on is that the Grey Silt was blown off fluviatile sediments along the Darling River and was trapped by the dense vegetation (such as the reed *Phragmites*) growing in the swamps.

Having been so trapped the calcareous material in the dust was in part eluviated down the profile of the Grey Silt. At its base, and in the top of the underlying unit of Black Swamp, calcareous nodules have formed — chemically aiding the preservation of bones and teeth in what would otherwise have been an acid black sandy clay.

Whatever the final explanation for this extraordinary dust may be, it is clearly a rare event. The underlying deposits of the Black Swamp show no sign of dust. In scale it is an event not to be compared with the trivial dust storms of today. The absence of soils developed within it suggests that it was a single event. If it was blown from the Darling River it perhaps represents either the onset of an arid period, with destabilization of alluvial materials previously stabilized by wet-loving vegetation, or it represents the occurrence of a flood of rare magnitude followed by a rare hot northwesterly wind.

That there occur floods with a magnitude vastly exceeding anything we have seen in historic times is implicit in Fig. 2. Again I have used Gumbel statistics, this time to estimate the return intervals for floods, of specified discharges, on the Darling at Bourke. The mind boggles at the implications, for deposits of alluvium on the flood plain of the Darling, of the flood with a return interval of 1,000 years.

Detailed sedimentological work must now be done on the Grey Silt and an attempt made to estimate its source and climatological significance. In many ways I would like to be able to interpret it as an arid event, because we could then link the aridity with the arrival of the Kartan industry — aridity pushing to the east people whose industrial traditions had, for some thousands of years, been stably settled well to the west.

Some answers to the environmental implications of the Grey Silt may come from a study of pollen spectra immediately before, during and after its deposition. Alas, no swamp deposits formed after its deposition at Lime Springs and Trinkey. Moreover

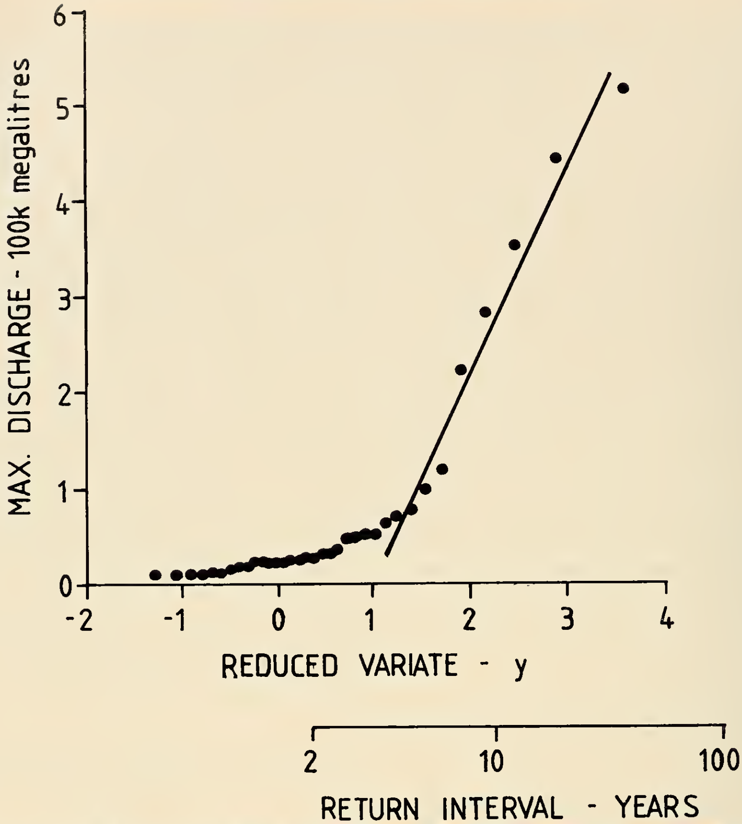

Fig. 2. Return intervals of floods on the Darling at Bourke. Note the implications, in the steepness of the line, for the magnitude of the thousand year flood. See text for relevance of flooding to origins of the aeolian Grey Silt unit at the site of Trinkey.

there is no pollen in the underlying Black Swamp unit. Pollen is to be expected in such organic-rich deposits, so evidently it was destroyed when the swamps dried 6,000 years ago, a process of destruction that would have been abetted by the calcareous contribution from the Grey Silt. I regard it as a primary objective of our field work to find a swamp that has remained damp and thereby preserved pollen from these critical prehistoric periods.

Meanwhile it is indeed tempting to rush in and explain the extinction of megafauna, on the Liverpool Plains, as 'caused' by the Grey Silt. However at Trinkey, underlying the Black Swamp, we have another dust, called the Buff Silt and tentatively dated to 26,000 years. The megafauna clearly survived this event — not only its environmental effects (whatever they may have been) but also the co-occurrence of humans at that time. For we have undoubted artifacts in the Buff Silt at Trinkey.

We can now seen an unexpected twist emerging from these two sites. Not only have we threatened the blitzkrieg hypothesis, but also the climatic explanation for extinction. For how can one appeal to an arid event at 6,000 years ago when an earlier event has survived unscathed? Moreover we have also threatened any hybrid model, which seeks to explain extinctions as a bit of climate with a bit of human interference, for humans were also on the spot when the dust blew 26,000 years ago.

Does all the work we have done leave us with no explanation for extinctions? Yes, for the Liverpool Plains. Certainly a crude explanation (in terms of climate, humans or both combined) will not work. I will therefore leave you with some speculation. Let us remember the important theoretical point, put forward by the proponents of humanly-induced extinction, whereby they ask what was new at the end of the Pleistocene. For the Liverpool Plains we must ask what was new after 6,000 years.

Two answers emerge. The first is the small-tool tradition, reasonably interpreted as an improved hunting technology. The other is the appearance of the dingo. Both events are thought to have happened around 5,000 years ago. It may well be hard to bring these events into archaeological focus and to relate them, in a mechanistic sense, to the process of megafaunal extinctions. Yet, over the next decade we shall seek to do just this in our field work.

As an audience you represent various disciplines in the natural sciences. I invite you to visit the critical sections we are establishing at our archaeological sites and to cast a constructive, if sceptical eye, over the diverse sorts of evidence we are uncovering. The question to which we are seeking answers is not scientifically trivial and certainly needs the attention of diverse experts. The cause of megafaunal extinctions is one of the most challenging ecological questions facing science in Australia.

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